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Revisiting Classic Qualitative Studies

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Abstract: This paper explores methodological issues regarding the revisiting of "classic" qualitative studies. Classic studies pose particular issues for secondary analysis. By virtue of being "classic", the findings and arguments of such studies define a subsequent "canon" of theoretical and methodological scholarship, and hence shape the thinking of subsequent researchers conducting secondary analysis. Secondary re-analysis therefore should be not only of the archived data itself, but of the published work itself, but this raises a host of complex methodological and ethical issues. Using my own reanalysis of Elizabeth BOTT's "Family and Social Network" archive, and John GOLDTHORPE and David LOCKWOOD's "Affluent Worker collection", I examine possible analytical strategies for re-analysis, including "debunking", the alternative of "sacralisation", and ways in which original data can be read "against the grain".

1. Introduction

For nearly a decade now, the potential for secondary analysis of qualitative data to be a distinctive new research tool has been heralded by social science methodologists (e.g. HAMMERSLEY 1997, 2004; FIELDING 2004; THOMPSON & CORTI 2004). Despite these clarion calls, there are still few substantive examples of how this kind of secondary analysis actually advances research. Many discussions concentrate on how particular researchers might re-interpret their own previously collected data (FIELDING 2004), though there

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are a few important exceptions.¹ Paul THOMPSON's (2000, p. 2) lament about the "reluctance to draw on material created by other researchers" remains true. In this paper I report on issues arising out of my own re-examination of two classic post-war social science studies as a means of exploring what contribution the secondary analysis of qualitative data offers social science researchers.

There are two main concerns about the secondary analysis of archived qualitative data. Firstly, given the impossibility of archiving the original and complete context in which qualitative studies were conducted, there are doubts about how researchers are really able to use such material to assess the validity of classic studies themselves. It is not clear that we can really assess the validity of the findings of generations of earlier researchers by checking their arguments "against" their data. As HAMMERSLEY (1997, p. 137) reports "making use of archived data is likely to be extremely time consuming, and it will not always supply what is required for coming to a conclusion about the truth or falsity of a study's findings". Secondly, it is not entirely clear how qualitative data can be used to address different questions to those posed by the original researchers. Given that the past studies inevitably address questions posed by past researchers, how much of the material is likely to be prescient to contemporary researchers? To be sure, archived qualitative studies data may contain considerably more information than was reported on in the published studies themselves, but it requires considerable investment to discover exactly what this consists of, and bring its potential for re-use to light.

These doubts and concerns are well rehearsed. In order to move these debates on, we now need feasibility studies reporting on how archived qualitative data studies might actually be used. This paper therefore considers how the archived qualitative data of two "classic" studies – both of which have become part of the British sociological "canon" – might be used with profit today. The two studies are Elizabeth BOTT's *Family and Social Network* (1957), and John GOLDTHORPE and David LOCKWOOD's *The Affluent Worker in the Class Structure* (1968/69).² I explore three points. Firstly, I examine ways in which the archived data might or might not be used to "validate" the studies themselves. I argue – in line with most current thinking – that this is a fraught exercise, which is only possible with certain limitations. Secondly, however, I argue that we can use the archived fieldnotes to gain distinctive insights into the research process itself. Archived qualitative data can be used to reconstruct how "classic" research studies were actually conducted so that we are better

¹ The most significant of which concern secondary studies of Mass-Observation data, which have achieved remarkable prominence in recent years (see for instance SUMMERFIELD [1988] and STANLEY [1993]). Paul THOMPSON's oral history interviews, the basis for his study of *The Edwardians*, have also been re-analysed (see THOMPSON 2000). In these cases it is striking that it is historians rather than social scientists who have been most important.

² In both cases, journal articles arising out of the studies were published as well as books. I make it clear which published works I refer to in the text below.

able to understand how research actually advances. Given the normative character of much social science methodology texts, where the focus is on how researchers *should* conduct their research, rather than how they actually went about their research,³ this offers an important, much underutilised, way of developing our methodological understanding. Finally, it is indeed clear that the published studies report only a small amount of the relevant data collected, and that there is significant potential for re-analysing material left for other purposes, but I stress that the means for doing this is not straightforward. Before moving on to discuss my two case studies, I need to situate them in the context of current methodological uncertainties.

2. The Secondary Analysis of Qualitative Data: A Methodological Impasse?

There is a strange modesty in calls for the value of secondary analysis of qualitative data. Whereas researchers proclaiming the importance of secondary analysis of quantitative data have been enthusiastic, even zealous (e.g. ARBER et al. 1988), those examining the prospects for the secondary analysis of qualitative data insist on putting all kinds of health warnings in place (for instance HAMMERSLEY 1997, in one of the first major statements in this area, or CORTI 2004). Or alternatively, as with FIELDING (2004), much attention is focused on disarming the objections of critics. Why? It seems that these concerns echo long-standing worries about the adoption of positivist frameworks for qualitative research.⁴ Most, though by no means all, qualitative researchers, reject the positivist concern with regularity, predictability and causal analysis through variable-centred research as inapplicable in the meaningful, human, social domain. However, since one motive behind the archiving of data might be based on a positivist concern to permit the replication of older studies, testing the reliability and validity of data, qualitative researchers are caught on something of a dilemma. If they subscribe to a version of an interpretative methodology, then the value of archived material, inevitably abstracted from the meaningful, intersubjective, human world from which it is derived, is doubtful. However, if they resist the call for the archiving of qualitative data,

³ It is worth noting that although social scientists have proved adept at deconstructing the research methods of contemporary natural scientists (e.g. LATOUR & WOOLGAR 1979; LAW 1994), their own practices have rarely been subject to the same rigours, though the practices of social scientists in the past have attracted more critical interest (e.g. YEO 1994).

⁴ The meaning of positivism is of course much contested (HALFPENNY 1982). Here I take positivism simply to mean the methods used, or seen to be used, in the analysis of quantitative data, whilst acknowledging that this is a crude labelling, though one which I think suffices for my purposes here.

their enterprise appears (increasingly) vulnerable when surveys and other quantitative studies cumulate, allowing replication and comparison on an ever more extensive scale. In addition, in an age increasingly concerned with accountability (ROSE 1999) not to archive looks like you might be trying to hide something.

One response is therefore to treat the task of qualitative analysis as residing in particular techniques for the analysis of data – just like quantitative techniques (see FIELDING 2004) – downplaying any link to interpretative methodology. In this case, the focus is simply on an examination of the data as collected, without reference to the researcher's experience in the field (see FIELDING 2004). Although this defence is a reasonable one, one wonders, however, how comfortable many qualitative researchers really feel in discounting their own experiences as contributing to their research. After all, the technique mentioned by FIELDING (2004, p. 100) whereby the actual analysis of qualitative data is done by someone other than the fieldworker remains very much a minority practice in the social sciences, which explains THOMPSON's lament that very few qualitative researchers do use other people's data. In addition, this approach defines the analysis of archived qualitative data as an adjunct of more established modes of analysis of quantitative data, thereby seeing it in subordinate relationship with quantitative research. It is this impasse that can be seen to lurk behind Martyn HAMMERSLEY's sensitive discussions (1997, 2004) on these issues.

Given this impasse, one major response has been to appeal to "history". Even FIELDING (2004, p. 104) who insists that we should not be sidetracked by epistemological concerns about qualitative research finishes by noting that "the discipline of history appears to provide the guiding premises in respect of some archival centres". Indeed, it is not co-incidental that the leading British academic who has championed the archiving of qualitative data, Paul THOMPSON, has strong historical interests (see e.g. THOMPSON 2000), and that most of the actual use of such material has been by historians. This is certainly the case with those who have used the life histories collected by THOMPSON as part of his *Edwardians* study. In part there is an understandable, indeed laudable, concern with the conservation of our research heritage. Regardless of how we can actually use the archived material of previous studies, surely it is still important to retain the data, as a means of allowing us to appreciate the studies themselves. Laments such as HAMMERSLEY's (1997, p. 137) that "it does seem extraordinary that there has been very little effort to preserve the mass of data that has been produced by qualitative researchers in the social sciences over the past 20 or 30 years" might best be read in terms of this concern with the conservation of our heritage. There is also the argument that because historians cannot be fussy and are forced to rely on the relics of the past, the documents that have, for one reason or another, been preserved, will – especially over time – take on the value of historical data (see also FIELDING 2004).

Whatever the limitations of the data may be, it is nonetheless better than any other available source, such as that derived from documentary sources (with their bias towards official accounts), oral histories (with their problems of recall), or surveys, (with their lack of sensitivity to meaning and context).

I think this appeal to history is fine so far as it goes, but it runs the risk of underselling the value of archived qualitative data, and it still lets positivist social science set the terms of the methodological debate. I want to suggest that one potential for archived qualitative data, which is consistent with an interpretative qualitative methodology, is to use it to show how we can understand the research process itself. Using archived qualitative data, we can unravel how the processes of abstraction inherent to research – qualitative and quantitative – are never neutral. This, in a sense, is the rendering of Walter BENJAMIN's (1973, p. 258) insight that "there is no document of civilisation which is not at the same time a document of barbarism". Rather than subscribe to the positivist aspiration that there really can be valid and reliable data, we might instead be able to show what kinds of constructions and omissions have taken place in the development of the social science canon so that we can read social science critically, not teleologically as the elaboration of ever more sophisticated and impressive methods which tell us more and more about the social world, but as about the ordering of both *visibilities* and *invisibilities*. This is an approach which challenges the dominance of positivist assumptions by allowing us to understand the implicit politics of social research which often gains its power and pertinence through keeping its own processes invisible.

These points are no doubt provocative, and for these reasons I now turn to my two chosen classic studies to elaborate these issues further. The two studies, both archived at the University of Essex as part of the Qualidata collection, are not chosen at random, but are especially pertinent because they were both formative in the elaboration of contemporary British sociology. Admittedly neither of them is a standard qualitative study, as they are often defined. As I go on to discuss, BOTT has a deductive and analytical flavour and she does not straightforwardly rely on an interpretative epistemology. GOLDTHORPE and LOCKWOOD's qualitative data was subsidiary to their survey based methodology, and indeed their study is more usually seen as an early exemplar of the potential of quantitative sociology.⁵ In fact this complex positioning of these two studies, as straddling the boundaries between qualitative and quantitative research makes them especially valuable for my concerns to explore the research process. In each case I briefly examine the long term significance of the study before concentrating on two related questions. Does a re-examination of

⁵ The two studies are archived at the National Social Policy and Social Change Archive at the University of Essex, referenced as UKDA study numbers, SN 4852-Family and Social Network: Roles, Norms and External Relationships in Ordinary Urban Families, 1930-1953 and SN 4871-Affluent Worker in the Class Structure, 1961-1962. Information about the collections can be accessed via the web site <http://www.data-archive.ac.uk/search/searchStart.asp>.

the archived data allow us in any clear way to evaluate whether the arguments of the authors are correct in retrospect? What does the archived data tell us about the research process itself, and in particular how studies which gained the status of classics came to be organised? I address a third issue regarding how the data might be used for other purposes today indirectly.

3. The Enigma of Family and Social Network

Elizabeth BOTT's study, *Family and Social Network* (1957), was derived from a large and unusual inter-disciplinary research collaboration based at the Tavistock Institute involving psychologists (led by A.T.M. WILSON), a social psychologist (J.H. ROBB) and a social anthropologist (BOTT herself). It was designed as a study exploring the relationships between psychological states, marital relationships, and the social situations of a range of urban households. BOTT's book, the only major publication to arise from this project, occupies a distinctive niche in the canon of British social science.⁶ Originating as a London University doctoral thesis, her subsequent monograph is regularly cited as one of the classic qualitative studies of post war sociology. Gordon MARSHALL (1990), in his account of the most important works of post war British sociology sees her study as his favourite, "the most original piece of sociological research to have emerged during the post-war era", because it "bring(s) to the study of society a profound understanding of the *interconnectedness* of social phenomena, and one which cannot be found in any other social science" (MARSHALL 1990, pp. 237, 236).⁷

In retrospect BOTT's study seems important as a pioneer of a standard conception of sociological research as a critical enterprise. In his defence of sociology MARSHALL argued that "Sociology, because it refuses to take social processes at face value is inherently sceptical. Governments deal in ideologies and pursue partisan objectives, for these are the very stuff of politics" (MARSHALL 1990, p. 235). MARSHALL here takes as the mission of sociology an idea which did not really exist in 1950, where sociologists, like most social researchers, were happy to start from the perceived problems of the day. The prevailing practice of social research in Britain until the 1950s had been primarily "problem based", focusing on what were deemed to be pressing social concerns: poverty and unemployment (as in the work of BOOTH and ROWN-TREE), housing and social conditions in various kinds of urban environment (MADGE, Ruth DURANT), family support networks (WILMOTT & YOUNG 1956), and the like. Here, the task of social research was to assess the extent of

⁶ All references here are to the 2nd edition, published in 1971.

⁷ In fact, MARSHALL's "claiming" of the book as a work of sociology rather than social anthropology is completely unfounded in terms of how BOTT saw herself (very much as an anthropologist). But this is a point which can be left for now.

the problem, find out its causes, and suggest what kinds of policies could address them.

The original research project undoubtedly shared this "social problem" problematic. It was animated by an interest in the dynamics of household relationships, and in particular in a relatively new post war problem – why some marriages did not endure and why divorce was rapidly increasing (from a very low base). However, the researchers took the novel step of seeking to address this issue not by seeking simply to conduct research on "failed", broken households, but rather by carrying out intensive research on a small sample of 20 "ordinary" households. The partners in these households were interviewed up to 20 times by BOTT and ROBB, subjected to psychological tests, and the households were subject to intense critical analysis. As BOTT put it in her PhD thesis "as the research went on, we became less interested in whether families were "ordinary" and more interested in how they worked as systems of social and personal relationships in and of themselves" (BOTT 1957, p. 11). Rather than seeking to explain why some marriage partners separated for some special reasons (with the implication that "normal" marriage partners stayed together and that no special reasons were needed to explain such "endurance"), the researchers sought out to understand the dynamics of household relationships in their own terms.

In addressing this concern BOTT argued that the relationship between husbands and wives in "ordinary" households could be understood in terms of the partners' social networks. BOTT contrasted the dispersed social networks of the professional middle class with the closer ties of the working class to those around them. She argued that the closer ties of the working class led both men and women to be more closely integrated into networks of their same sex peers, with the result that there were more segregated role relationships between husband and wife than was the case in many middle class families. From this insight, the core idea of emphasising the "connectedness" of individuals and households to others became popularised, and helped to generate an influential account of working class families and community life. Her study has subsequently been seen as a seminal source in the development of social network analysis, which has now become a leading social science method in its own right, especially in the US, Canada, and parts of Europe (see SCOTT 1990).

Looking back, we can see BOTT's study of social network as a prime example of the kind of generalising and abstracting social science that dominates mainstream conceptions today. Its concern with developing a formal account of how social networks shape intimate relationships has echoes of DURKHEIM's account of suicide. We do not need to attend to the motives, values or ideals of individual agents, so much as the social relations in which they are embedded. Thus, the celebrated anthropologist MAX GLUCKSMAN (1971), in his preface to the second edition to BOTT's book, used her case studies to celebrate the

potential of network theories to unravel the transition of African peasants to the town, for instance.

What do we gain by returning to the archived fieldnotes for this study? Let us begin with the issue of whether we are in a position to assess BOTT's own arguments about the relationship between social networks and the domestic relationships of the marriage partners. As a preliminary it should be noted that the data archived in Qualidata almost certainly consists of only a small part of the data generated by the wider study itself, mainly a series of typed case notes on most of the households, synthesising the views of the researchers on the households in question. These are usually between 20 to 30 pages in length, reporting on general points about each household. Notes on each of the specific interviews carried out with the respondents (assuming they were taken) have not been retained: we therefore have the interviewer's gloss on the interviews, rather than the words of the respondents, reported verbatim. In addition, data on the psychological tests do not appear to survive. From the limited data which does exist, it is clear that we have absolutely no prospect of "testing" BOTT's arguments. Very little of the fieldnotes is actually about the social networks of respondents, but concentrates on psychological aspects of the marriage partners, their work and neighbourhood relationships and their reference groups. Admittedly, the family ties of all the households were, at least for some households, fully delineated, with up to 156 family members enumerated and at times graphically represented. In addition there was some discussion of neighbours and friendship networks. But, a great deal of her reported analysis was based on her intensive and tacit knowledge of the respondents and is not now amenable to be "checked" against the existing data.

In many respects we should not be surprised. The study itself was not set up to examine social networks, and as BOTT clearly states, she only came to recognise the possible significance of social networks well after the interviews had been conducted in 1951-2. This interest arrived inductively as a means of trying to make sense of already conducted interviews,⁸ and it is not surprising that the fieldnotes were not systematically organised to record the networks of respondents themselves. So, let us turn to a second question. What do the fieldnotes tell us about the research process itself? And here, they do help shed light on an enigmatic quality of the study. Despite her pioneering role, BOTT herself never subsequently played any role in developing network analysis after the publication of *Family and Social Network*, and lost interest in the method shortly after completing the book. Indeed, despite playing such a critical role in the development of post-war sociology, she ended up "switching ships" and

⁸ It is not entirely clear when exactly BOTT developed her network approach. She appears to have developed it by late 1954, when she gave the seminar to the Manchester anthropologists who were "almost as pleased with the idea as I had been" (BOTT 1971, p. x) but presumably it was after her seminar on class as a reference group to the Cambridge anthropologists.

spending her subsequent career conducting and writing about psychoanalysis. She never repeated the kind of empirical enquiry that marked our *Family and Social Network* as a classic study.

Given her own career trajectory, it is interesting to note that scrutiny of the research fieldnotes written by ROBB and BOTT suggest that what really pre-occupied them was the psychology of the couples. Much of the material in the fieldnotes were attempts to make psychological sense of the respondents, using the social-psychological theories of the day. There are a lot of references to "hysterical" or "domineering" women and "impotent" men in these notes. To give just a few examples:

"Mr F's control and domination of his wife in various ways is no doubt a more loving and tolerable form of her mother's domineering ways. In escaping from her mother she has not lost the security involved in being controlled" (THORNTON).

"While his peaceableness seems to be largely a kind of obsessional attempt to keep order and control in a world that threatens to get out of hand, I think hers is more of a depressive reaction to a situation which seemed to offer extremely limited opportunities for satisfaction" (SALMON).

"The most striking thing about him was what can roughly be described as some kind of creative urge. He likes to make things and to see them as working, linked to his interest in film projection and drama ... insofar as he is deviating from full masculinity he seems to be more like a boy openly and unashamedly looking for a feminine mother" (JEFFRIES). (Manuscript source, see Note 6)

These notes might be attributed to ROBB, who was more greatly involved in the household interviews, and whose background was in social psychology. Certainly, this would be consistent with how BOTT reports the research process: "Robb had a more eclectic approach (than Bott) and was less pre-occupied with the difficulties of modifying and integrating sociological and psycho-analytic theory" (BOTT 1957, p. 31).⁹ ROBB's fieldnotes, especially those early in the fieldwork, certainly indicate that he was actively interested in interpreting aspects of the material in psychological terms, especially with respect to their respondents' relationships with their extended family and with each other.¹⁰ At times he took it on himself to dispute the psychological interpretation of the respondents made by the psychologists themselves.

⁹ In fact this quote is from the methodological introduction which ROBB co-authored, but as the file of correspondence between them (which is on file in the Qualidata notes) indicates, ROBB himself did disagree with BOTT on several issues.

¹⁰ Indeed, although referred to as a social psychologist, and with some expertise in sociology, ROBB was clear that his expertise here was limited "I know my LSE degree was nominally in sociology but apart from a nodding acquaintance with Weber (which I suspect you feel didn't do me any good, ideal types and all that) I really didn't learn much about sociology until I came into contact with you and Homans... The only systematic theory that I know about in social psychology that I knew about was culture and personality. I think you rather

Yet there is more to the matter: it would be wrong attribute this character of the fieldnotes simply to ROBB. When the study started, the key issue was to understand how personality was related to household dynamics. This was not simply an issue for the psychologists on the research team – much social theory of the day was preoccupied by the social organisation of personality, and argued that social relations were important through influencing personality.¹¹ BOTT later claimed that she refused to entertain this "psychologisation" of social relationships at the outset: "she interpreted almost as a personal affront suggestions that personality factors might provide the answer" (BOTT 1971, p. 31). However, a careful reading of her work suggests that this was not quite true. Compare these two rather different ways of thinking about the relationship between social relations, personality, and behaviour: "Bott had a preference for attributing behaviour to social causes rather than to individual personality factors... At this time she was trying to explain segregation of conjugal roles in terms of differences in occupation and neighbourhood" (BOTT 1957, p. 31) Elsewhere, however, BOTT (1957, p. 33, *italics mine*) remarks that

"she had been convinced that actual behaviour was somehow *a synthesis of personality on the one hand* and a fixed, immutable social environment on the other. She moved towards the view that the external social environment permits much choice, and within broad limits individuals can construct their own environment in accordance with their own conscious and unconscious needs".

She was also clear that she was interested in "fitting together the psychological and sociological analyses", and subsequent parts of her book continue to rehearse the possible significance of personality explanations (see BOTT 1971, pp. 109ff). She therefore seems to have been ambivalent about whether she wanted to resist psychological interpretations, with their attendant concerns with individual personality tout court, or to relate them more clearly to social relationships.

This point suggests a way of re-interpreting her study. BOTT's account of social networks actually grew out of the failure of the project as originally conceived. BOTT's collaborators never wrote up their psychological material in any but the most cursory way, and as BOTT herself put it in a letter to ROBB, as she was about to complete her monograph, "the ambitious scheme of integrating the sociological and psycho-analytic analysis has gone by the board" (26th April 1956). There is no reason to doubt the sincerity of her conversion to the social network approach, but one can still observe that this had the advantage of allowing her to salvage part of the project: she could write up her part of the project without treading on the toes of the psychologists whose task it was to write up the psychological sides. Network approaches repositioned sociological explanations so that they did not need to rely on an account of

flatter me in suggesting that I used 'role' in the sociological sense" (letter from ROBB to BOTT, 1956, box 19).

¹¹ The early issues of the *British Journal of Sociology*, for instance, have numerous contributions from psychologists, including the young H.J. EYSENCK.

personality, which could subsequently be seen as the province of psychology alone. Sociologists did not need to give their own account of personality, different from (whether in conflict with or compatible with) psychologists, but could instead develop explanations that had no need to call on a theory of personality. The irony, of course is that she herself was not persuaded enough by her own argument to retain a prime interest in social science and later trained as a psychoanalyst.¹²

There is a further point to make here. BOTT's research may have been pioneering, but it also marked the end of a long tradition of social research based on the casework approach. The fieldnotes indicate how closely the project was allied to the tradition of Victorian social work, concerned with evaluating the moral capacity and household circumstances of particular families. An adjunct to this is that the researchers were quite happy to pass judgements about the respondents but – so far as I can tell – did not communicate these to the respondents so that they might respond to them. In some ways, the study can be seen as the final flowering of Victorian social research, with its fastidious concern to unravel household relationships, and their moral calibre. Today, this kind of moralising project seems completely out of place, even unethical. BOTT's study marks not only the development of a new kind of social science, but it also marks the closure of an older one. And, this closure was due to serendipity. The fieldnotes today allow us to see that BOTT's pioneering study arose out of the broader failure of the project on which it was based.

4. Revisiting the Affluent Worker Study

The Affluent Worker study has, like BOTT's, become part of the sociological canon, and because all of its authors went on to have distinguished careers, its influence is in many respects even more marked: Gordon MARSHALL (1990, p. 112) notes that it is "probably the most widely discussed text in modern British sociology" (and see also PLATT 1984). Its arguments (GOLDTHORPE & LOCKWOOD 1968a, 1968b, 1969; GOLDTHORPE & LOCKWOOD 1963; LOCKWOOD 1966) became the standard orthodoxy not only amongst sociologists, but also in other academic disciplines and amongst political commentators and critics. Its core claim, that the growing affluence of sections of the working class does not entail the end of class division, or necessarily of class politics, but that class remained a central feature of British life even in a prosperous, consumer society, had had a profound impact in shaping subsequent debates. This intervention is one reason why the British interest in class was

¹² The reasons for this are complex and include her concern to take an occupation which allowed her to work part time when bringing up children (see BOTT-SPILLIUS forthcoming).

sustained when in many other nations the study of stratification in prosperous society generally led to the eclipse of class as a source of academic interest.¹³

Why has the study been so important? In the context of the rapid expansion of the discipline of sociology in the 1960s, the study acted as a template as to how the nascent discipline of sociology could comport itself and carve out a distinctive role vis-à-vis its better established neighbours in the social sciences. Building on BOTT's foundations (to whom the authors acknowledged their debt), this involved repudiating the "social problem" problematic. One can immediately see the subversive appeal of the idea of studying affluent workers. No one conventionally thought that the existence of increased prosperity amongst manual workers in the new assembly line industries was a social problem: indeed the reverse. To be sure, a political problem – whether the working class electoral base of the Labour Party would decline as a result of affluence – had been identified by political commentators (especially ABRAMS 1960). GOLDTHORPE and LOCKWOOD were clearly keen to engage with this public debate about the significance of the affluent workers for class alignments and political change, but on their own, explicitly sociological, terms. The real problem which animated them was the challenge that affluent workers posed to different versions of sociological theory. A dramatic feature of their inquiry was the way they commenced their final volume not by commenting on how their study derived from social problems or political issues, but with a very clear statement of the theoretical starting point of their concerns: "the debate on the working class... has its origins in the work of Marx and Engels" (GOLDTHORPE & LOCKWOOD 1969, p. 1). This was a trumpet call for sociology, and for sociological theory, to define pressing questions for research, and in the process to rework what the public, or politicians, might assume to be the "social problems" worthy of social investigation.

As part of this concern, secondly, they used deductive framing in their writing up of their study. The fundamental character of this approach was to pose the kinds of empirical findings that might be expected from theoretical assumptions (through the use of WEBER's ideal types), with the research findings being used to measure the gap between people's accounts and what would be expected from these theoretical models. GOLDTHORPE and LOCKWOOD were absolutely clear about the way that the interviewees did depart from theoretical assumptions. This was especially true with the argument that workers had instrumental orientations to work (especially in Volume 1), a point which only occurred to the researchers after they had conducted the study. The point here is that GOLDTHORPE and LOCKWOOD did not report their findings inductively, but deliberately sought only to look at evidence which allowed them to confirm or disconfirm their theoretical hunches.

¹³ Especially in contrast to American research, for an overview of which see GRUSKY 1996.

Finally, they were concerned to use distinctively sociological methods to explore the situation of affluent workers. In many respects, GOLDTHORPE and LOCKWOOD's study was like many others that had been carried out by previous generations of social researchers, in combining qualitative and quantitative methods through detailed case studies of community and work relations. However it broke new ground in its emphasis on reporting its findings through relying on predominantly quantitative measures. The study reports the findings from survey data, based on cross-tabulations derived from the two separate surveys carried out in the workplace and at home (with husbands and wives present). In fact, these findings would not be accepted as convincing quantitative evidence by contemporary standards: the sample size (229 households) is small, and levels of statistical significance (which were in any event not reported) would probably not have been achieved for most of their findings. In addition, rather than being a random sample of a particular workplace or locality, the total of 229 cases is actually comprised of six separate sub-samples drawn from different occupations in three firms. Thus, the arguments of probably the most influential quantitative British study would be regarded by nearly all referees of contemporary social science journals as fundamentally flawed because of their reliance on inadequate data.

An example of the novel character of their approach can be found in their account of the respondents "images of class". Rather than trying to report these inductively by identifying the motifs most commonly mentioned, they constructed this exercise as a means of seeing how the respondents might be fitted into the model developed by LOCKWOOD (1966). Building on BOTT's (1957) claim that talking about class is a means of exploring symbolically experiences of power and prestige, LOCKWOOD (1966) had argued that workers might be expected to have one of three images of class. In a power model, they saw society as divided between two classes – a "them" and "us". In a prestige model, they divided society into three groups, each with a different position according to their place in a status hierarchy. Finally, in the pecuniary order, society was divided into a graduated hierarchy with no marked breaks, since people's position was defined by their financial situation, which itself could vary in slight ways from those of others. The research therefore was concerned mainly with fitting respondents into this typology derived from sociological reasoning.

Let us now pose our two main questions. Firstly, do the fieldnotes allow us to validate the arguments of the study? On the face of it, yes. All the relevant questionnaires have been archived, and we can be confident there is no missing data. The male respondents were interviewed twice, once at their workplace, and once at home (with their wives present). It would be possible to re-read all these interviews to examine the accuracy of the coding, and possibly to add extra codes to those which were listed by the researchers. It would also be possible to examine the discursive material which accompanied particular

questions to assess whether this might in retrospect offer a different perspective. This is certainly possible for the sections of the household interviews, which were very full, sometimes lasting for four or five hours, and where, although a structured survey was used, numerous open questions were also asked. In the case of the questions about class identity, what stands out is the freedom with which interviewees were given to talk about class in any way they wished, without particular prompts from the interviewer (see generally here, the pertinent comments of BECHHOFFER 2004). Starting from an initial prompt: "People often talk about there being different classes – what do you think?" the interviewers then led into an open discussion.

"The schedules had printed on them a check list of issues on which interviewers were to seek to establish the respondents own views, but the order in which issues were raised could be varied, following the natural flow of discussion, and interviewers were instructed to try so far as possible to formulate their questions *in ways consistent with what they had already learned about the respondents' ideas and conceptions*" (GOLDTHORPE & LOCKWOOD 1969, Appendix C).]

Where husbands and wives internally disagreed about their views, their differing views were normally recorded separately. The subsequent discussion often led to several pages of written notes, with verbatim quotes of up to 2000 words.

Looking back, it is clear that these responses amount to the most detailed qualitative study of class awareness and identity that has ever been undertaken in Britain. Yet, because of the new, deductive, sociological approach championed by the authors, only five pages of their book considered their findings (though see GOLDTHORPE 1970 for a somewhat more extended treatment), with little attempt to convey the flavour of their respondents' testimony through quotation. The authors found the richness of the material difficult to handle in a formal way, and were concerned that different readers would interpret this material in varying, possibly inconsistent ways. They hence devised an original means of coding the data. Only where there was a high degree of consistency between different coders was the material coded, and then its broad findings reported. This normally meant ignoring the more qualitative features of the interview and concentrating on those aspects of the respondent's testimony which could be quantified – for instance the number of classes which respondents identified (see GOLDTHORPE & LOCKWOOD 1969, Appendix C). In the process, a huge amount of evocative material was left "on the cutting room floor". Having gathered rich qualitative material, the researchers explicitly then stripped out such materials in favour of more formal analytical strategies.

This does give us the potential of exploring whether the discursive material appears consistent with the arguments developed by GOLDTHORPE and LOCKWOOD on the basis of their ideal types. GOLDTHORPE and LOCKWOOD were of course scrupulous in noting that respondents images of social class "exhibited a considerable amount of diversity ... respondents were some-

times rather vague and confused in their formulations" (GOLDTHORPE & LOCKWOOD 1969, p. 147). Indeed they frequently noted the incoherence of people's actual images of class. The issue, however, is whether this apparent incoherence is because the views expressed by respondents did not fit the models that the sociologists were using, rather than because they were incoherent in their own terms. Let us now consider whether a re-reading of the verbatim interview material might permit a different perspective on the images of class revealed in this study (see further, SAVAGE 2005).

Let us start, with the assumption, ultimately derived from BOTT, that people's images of class are means by which they symbolically talk about their experiences of power and prestige. This does not seem to tally with the way that respondents talked about class. With very few exceptions, respondents had heard of the concept of class, and could articulate some kind of view about it, but – as GOLDTHORPE and LOCKWOOD freely acknowledged – their responses were frequently hesitant and inconsistent. Only for a minority of respondents was the idea of class important in giving them a sense of their own identities. Indeed, it was particularly the questions about class which evoked puzzlement and confusion, even for interviews which were otherwise clear and direct. An example is the interview notes for respondent 57, which recalled that he was

"very intelligent and lively ... Seemed to sense what kinds of information particular questions were seeking. The direct questions he answered quickly and would look up as if to say 'what's next?' – this contributed a lot to getting as far as the politics section in about 1½ hours. However, on class he got out of his depth, thought too hard, and in fact said most of what he could say spontaneously on the enumeration section. As the section wore on, his answers got slower and more uncertain. I think he was rejecting a lot that came into his head as not sufficiently well thought out. Towards the end he was openly exasperated, but in a good humoured way, and just said he couldn't find anything else to say" (Affluent worker archive, no 57, see Note 6).

This hesitancy does not appear attributable to lack of intelligence or being uncomfortable with an academic interviewer (though there were other cases where that was clearly true). Rather, he seems to be aware that the concept of class has social and political currency, and that there is some kind of "right answer", and his hesitancy arises from his perceived inadequacy in providing this. Rather than the concept of class being one which tells someone about their own place in society according to their own social relationships, respondents rather see it as a way of describing social divisions "out there", in society at large, but not as one very relevant to their own identities or social position. This explains why some respondents rooted class in historical divisions (for instance arising out of the relations between lords and serfs), or as being characterised by divisions between a visible upper class, and what might be deemed "ordinary" people. Class was a language of political and social description, where respondents understandably varied in the kinds of cleavage and divisions

they unravelled, but they did not, by and large, see this as an important means of making sense of their own situation in society.

GOLDTHORPE and LOCKWOOD (1969, p. 149) argue that amidst the diversity in responses, there was one general finding about people's images of class, which was that money was the most important determinant of class, and

"furthermore, virtually all those who held this idea were also alike in one other respect: that is in seeing as a major feature of present day society a large "central" class which embraced the bulk of wage and salary earners and to which they themselves felt they belonged".

GOLDTHORPE and LOCKWOOD interpret these findings to indicate that their sample of manual workers have neither a power-based model (distinguishing between "them" and "us"), nor a status based conception of the social order (distinguishing clearly between classes differentiated by lifestyle and status position). They see the central role of money in the views of the affluent workers as entailing a lack of a clear awareness of a power divided class society.

A reading of the interviews allows one to question this view. Respondents did not see their reference to money as entailing a graduated view of society. Rather, they saw money as dividing people on power and class lines. Now, although GOLDTHORPE and LOCKWOOD (1969, p. 149) recognise clearly enough that most of those who have a money model of society also think there is an upper class – "an elite strata whose economic superiority was such as to give them a qualitatively different position", they do not note that money was often seen by respondents as intimately linked to power. Differences in money between an elite class were frequently seen as the result of power differences. To take one example, No 47 reported disapprovingly the existence of a "toffee nosed" upper class, and also a middle class of "people who can afford a house in the £3000 to £6000 bracket – they have a little bit more money than the ordinary working man – people like departmental managers and things like that" (Affluent worker archive, interview number 47, see Note 6). He then characterised the working class as "a bloke who earns anything from £8-10". Such an account at one level sees class in terms of money, but it sees these monetary divisions as quite consistent with marked class divisions. Indeed, No 47 went on to note that money was the only difference between classes, and that boundaries between classes were indistinct. Then, later in his discussion, No 47 develops his account, noting that he sees himself as working class because "its his type of job – just an ordinary job on the shop floor". (i.e. there is a group of "us"), and that the rich are rich because "I think it was handed down really – handed down from father to son – rich families marry each other like – in horse racing jockeys and training families marry each other". We do not need to read this account as an incoherent or ambivalent one. Rather, it seems utterly clear. But because GOLDTHORPE and LOCKWOOD see money models of society as analytically different from power ones, they do not register this consistency. Many respondents see the differences between classes are funda-

mentally financial ones, but the causes of these are related to the exercise of power and inheritance.

The distinctiveness of this way of thinking can be revealed by contrasting it with a very few respondents where there was a much clearer sense of a graduated pecuniary model of society. Although GOLDTHORPE and LOCKWOOD do not highlight the views of the significant minority of immigrants in the sample, it does seem that these subscribe to the kind of money model that GOLDTHORPE and LOCKWOOD have in mind. Thus one Cypriot immigrant talked of thousands of classes, each with their own income, so that "each man is in a different class" (No 45). One way of noting the importance of this point is that the majority of the respondents (53%), when asked to talk about class, talked first about the "upper class" before mentioning any other class, and that the vast majority identified an upper elite class in their accounts. Only 5% of respondents did not talk about the existence of an upper class of some kind, and many respondents talked about this upper class with more clarity and vigour than any other class. To this degree, rather than there being any confusion about class, there was remarkable unanimity. Although this upper class is given very different names, it clearly differentiates a small elite from the "average" person. It is a public, visible, class whereas most people are relatively private; it is a class which does not have to work, whereas most people do; it is a class where money is abundant, whereas most people have to watch carefully; it is a class whose position is based on inheritance, whereas most people have to make their own way.

This very strong and clear identification of an upper class which combines having lots of money, with high status, public visibility, power and social connections suggest that GOLDTHORPE and LOCKWOOD's (1969, p. 146) claims that "few saw society as being divided into two confronting classes on the basis of the possession or non-possession of power and authority" can be seriously qualified. In fact, nearly all the sample did identify a clear class division between a rich upper class and a broadly defined "rest", and saw power as one factor which was intrinsically related to this central division. GOLDTHORPE and LOCKWOOD are clearly correct that most respondents did not clearly differentiate middle from working class, or white collar from blue collar. Very few respondents distinguished classes on the basis of workplace relationships. However, respondents' views could be seen as eminently compatible with a kind of Marxist differentiation of a bourgeois class from a large working class including manual and routine non-manual workers. This sense was often linked to clear statements about the inequity of these arrangements

"I think the very rich class keep the money in their own families". (No. 1)

"[T]hey have money and don't have to worry ... Top class people very cliquey, group a lot, stick together". (No. 2)

"Money is nearly 90% of everything, it's more or less left to them, from father to son, from business and everything". (No. 3)

"[S]ome people thiefe it; some of those rich people, why are they rich? Because they hold thousands of acres of land that don't belong to them, thieved years ago and handed down. The duke of Bedfordshire, what right does he have to half of Bedfordshire? Hundreds of years ago they fought – 'let's have a fight here' ... if people couldn't pay taxes, that land was taken from the people, that how these big estates come about" (No. 34). (Affluent worker archive, see Note 6)

Returning directly to my point about how we might use the data to validate the study, these observations suggest that whilst we can say that the data probably is consistent with GOLDTHORPE and LOCKWOOD's interpretations, this does not mean that their interpretations are necessarily the best ways of interpreting the data. There is the potential here to use other research strategies today to suggest alternatives.

Let me now turn to the second question regarding what we learn about the research process from using the archived material. I have argued that GOLDTHORPE and LOCKWOOD's study promoted a tripartite strategy which defined a new kind of sociological expertise: (i) drawing specifically on sociological theory (which was hence concerned with sociological, rather than social problems), (ii) relying on formal reporting procedures involving codification and quantification of data and (iii) a deductive approach to social investigation. Yet I have also emphasised that because the study occurred on the cusp of the transition to a new framework for sociology, there are ample traces of an older inductive strategy all too evident in the archived interview schedules. Rather like BOTT, we can see that GOLDTHORPE and LOCKWOOD not only opened up a new way of doing sociology, but they also closed down an older version of social research, where interests in inequality and stratification were rooted in community studies. Their archived data amply show their interest in Luton as a "community". All the addresses of the respondents were plotted in terms of their social geography, and comments made about the nature of their local area and its social ecology. Each of the household interviews has full notes about the décor of the house, its arrangements and furnishings, which indicate a much more ethnographic interest than is apparent in the publications. None of these more ethnographic aspects were ever written up, and their account now reads as a study of a group of affluent workers abstracted from their place of residence. A similar process can be seen too in the workplace interviews, where contextual observations about their work were never reported in the study. The success of the study rested in part on making invisible certain features of the lives of their sample. GOLDTHORPE and LOCKWOOD were of course admirably frank about this, since it was the direct result of their deductive strategy, but we might today look at the wealth of material collected and consider what has thereby been lost.

5. Conclusions

Let me now take stock of our findings by considering the broad lessons that we learn from our two case studies about the potential for archived qualitative data. Firstly, we have examined some of the ways that fieldnotes can be used to evaluate the arguments of the study's authors themselves. In the case of BOTT, the archived fieldnotes only appear to be a selection of some of the data collected by the study, and in any event her arguments were based – at least in part – on her detailed ethnographic knowledge of the households that was probably never written down, and certainly has not survived. The *Affluent Worker* study is more interesting in this regard. All the relevant data appears to be available, and because it is possible to reconstruct how the researchers coded it, it is possible to tell whether their arguments are justified in terms of their own data. There is little reason to doubt that the fieldwork data is largely if not entirely consistent with the arguments developed by GOLDTHORPE and LOCKWOOD, but such consistency depends on their limited analysis of the data as a means of adjudicating between ideal types. To this extent, the *Affluent Worker* study certainly has the potential for profitable restudy along the lines indicated by FIELDING (2004). However, this potential is largely present because of the fact that this is a very unusual "qualitative study" in the first place, because the qualitative aspects of their research were subordinated to a survey methodology. Thus, it seems that the more that qualitative research is fully qualitative, the less easy it will be to replicate and validate the study itself.

Secondly, we learn a great amount about the research process from the fieldnotes of these two studies. In both cases, we can see that the actual research process appears as rather more complex than the post-hoc accounts of the research as written up in the finished volumes. BOTT portrays her study as one in which her own anthropological and social perspectives competed with those of psychologists, but it actually appears that she too had real interests in the psychological aspects of the study, and that the arguments about networks were a means of rescuing part of the project from a broader failure. GOLDTHORPE and LOCKWOOD wrote up their study as a deductive account, but in fact it began in much more inductive fashion, with few concrete ideas and a more general interest in the topic of affluent workers. In neither case did the authors misrepresent their work, but they simply did not mention certain aspects which appear to have been pertinent at the outset of the inquiry that we are able to piece together in part using the archived qualitative data. In both cases, we see research which has come to be regarded as "classics" did not follow a standard social science methodology where research questions are firstly delineated, then appropriate methods chosen, the fieldwork done, the data analysed and the results written up in a kind of linear process. In fact, in both cases, the early motivation appears mainly to be a broad interest in a topic, and the subsequent theoretical framing is derived post-hoc. What we see is how the process of

abstraction, which is necessary for a piece of research to endure, involves reading the data in particular way. By returning to the archived qualitative data we can expose some of these absences.

We are also thus able to see how these two influential studies were important in part not only because they opened up new lines of inquiry, but also because they closed down others. Certain issues are made invisible as a condition for making others visible. So, BOTT's observations about the social networks of her 20 households also involve the neglect of the vast amount of material she and her colleagues collected on the psychological and personality factors of the respondents. GOLDTHORPE and LOCKWOOD's focus on class and work entailed downplaying neighbourhood and community relations. It is not simply that we know more as a result of major, classic, studies, but rather that we know differently. Making certain things visible entails making other things invisible.

Thirdly, we are able to use archived qualitative data to ask new questions. As we have shown, however, this is not to say that the data can be used as a neutral resource, since we always have to bear in mind the research process by which the data is collected. And of course, this is a lesson for all secondary analyses of any kind of data, including quantitative survey data. But here we can see a real advantage for the secondary analysis of qualitative data in that the research process is less easily written out of the archived data than is the case for survey sources, where subsequent researchers usually just have access to the codebook and the data set. For these quantitative sources, the abstraction process is often so complete that the traces of the original fieldwork have been altogether covered over. This process of covering over the traces of the fieldwork is much more difficult for qualitative research, and this is a fact which should be celebrated, rather than seen as a "problem" (as it might be from within a positivist perspective). This kind of secondary analysis of archived qualitative data gives the potential to read history "against the grain". We have the possibility of returning to these classic studies, with a view to asking what questions were lurking which were never elaborated in published reports, and to recognise the coding of certain absences. It allows us to use such data, using contemporary analytic techniques, to expose new issues and question established wisdoms. It allows us to see what is lost in the process of the "advance" of social science research.

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